By the above experiments we have endeavored to prove the invalidity of the claims that colored shadows must necessarily be explained as an effect of deception of judgment, and that they are therefore a proof of the psychological nature of color contrast. We leave it still an open question whether contrast is psychological or physiological in its nature. Hering's formulæ seem to apply to it as well as do those of Helmholtz. Colored shadows throw no light on the controversy; for the experiments with colored shadows, which have been supposed to prove conclusively the psychological theory, are experiments not in color-contrast but in retinal fatigue. The unwarranted claims of the "psychologists" have resulted from the fact that hitherto care has not been taken to distinguish between the two phases of the shadow—that of pure contrast, which ordinarily is never seen, and that due to fatigue by the prevailing light, which begins at once over the entire retina, since the eye keeps moving continually over the field. Helmholtz waited for this effect before performing his tube experiments, as we notice by his direction to allow the blue of the shadow first to become intensely developed; and then he wrongly called the result an effect of contrast. His followers have done the same. Our positive result, besides the separation of the two phases of the shadow, is this: We have seen that the color of the shadow persists in the experiments with the tube only when it is due to retinal fatigue; that its long continuance is paralleled by an experiment in which the persisting color is an after-image which is not caused by "unconscious anterior judgments" induced by the belief that we gaze through colored media; that the color gradually disappears, a fact which cannot be accounted for by the psychological theory; that when it disappears suddenly by comparison with the grey field outside, it is because the sensation is thrust under the threshold, or has reached the stage where it is more imagined than seen; and that all the other phenomena observed are entirely compatible with a physiological explanation—some of them being thus even better accounted for. These facts, we believe, entirely disprove the claim that only a psychological explanation of colored shadows is possible, and make some physiological explanation at least possible—perhaps even absolutely necessary.

EDMUND B. DELABARRE.

Ueber den Grund der Abweichungen von dem Weber'schen Gesetz bei Lichtempfindungen. H. EBBINGHAUS. Archiv f. d. ges. Phys. Bd. XLV.

The researches of König and Brodhun, more rigid than any that have hitherto been made (this JOURNAL, Vol. II, p. 330), as to the degree of exactness with which Weber's Law applies to sensations of light-intensity, over a wide range of variation of intensity, and for six different points of the color scale, have induced Ebbinghaus to endeavor to account for the fact that departure from the law is the rule, and that its exact applicability holds only over a small range of medium intensities. Without attempting any explanation of the fact that the intensity of a sensation increases as the logarithm of the intensity of its objective cause, he endeavors to show why that should *not* be the case at the two ends of the intensity-scale, and in the following way. Assuming that the process which goes on in the retina is of a chemical nature, it is necessary to apply to it the conceptions that are current in modern chemical theory. The chemist has given

up the idea with which he started out, of the absolute uniformity of the substances with which he deals. As the physicist now identifies the pressure of a gas confined within a given volume with the *average* energy of motion of particles which are flying about with countless different velocities, so the chemist is now forced to look upon a given chemical constitution, not as a fixed and homogeneous, but as a *mean* state of aggregation of a given lot of atoms or groups of atoms. (B. Horstmann, *Theoretische Chemie*). When any agent is applied whose effect is to produce increased dissociation, it will have a fixed *average* effect upon the whole mass of matter concerned, but its actual effect upon any individual molecule will depend upon the condition of that molecule before the new force was applied. If it was already so *loose* in structure (to use a word for which there seems to be an absolute necessity), that is, if the atoms which composed it were so remote from their mean center and performing such rapid oscillations of their own that the force applied was just sufficient (or more than sufficient) to break its bonds altogether, then that molecule would be destroyed. A molecule which was of firmer constitution would not be destroyed by that particular application of energy, but would simply be brought nearer to a state of dissolution than it was before. If, now, the disgregation conditions of the different molecules occurred in accordance with the usual law for distribution about a mean, much the greatest number of the molecules would be in a state of disgregation near the mean, a gradually diminishing number would be of looser, and also a gradually diminishing number of a less loose constitution. Consequently, the effectiveness of any applied force (the light falling upon the retina in the case before us) would be greatest if it were just capable of breaking up the molecules near the mean. If it were of a less intensity, there would be a relatively smaller number of molecules which it was capable of destroying; if its intensity were increased, there would again be a relatively smaller number of molecules left to be destroyed. This agrees in sense with the departures from Weber's Law; a low intensity and a high intensity of light are both less effective than that law would require them to be. Ebbinghaus attempts to show that there is a very exact correspondence between the departures exhibited by König and Brodhun, and those required by his theory. But in determining the actual distribution of disgregation states among the molecules, he transforms the kinetic energy of each atom into potential energy and then counts up the influences which affect, not its actual kinetic energy but its equivalent energy of position. This latter proceeding is plainly unjustifiable. The coincidence which he obtains between theory and fact seems to be, nevertheless, remarkably close.

It is a very curious proceeding in scientific method to attempt to account for the deviations from a law, when there is absolutely no explanation of the law itself—no physical explanation, that is; Mach's explanation is of a teleological nature. It seems to us to be, however, in the present instance, a proceeding which is not wholly illegitimate. The explanation itself rests, of course, upon the assumption that the relativity of Weber's Law is an objective fact, and not an affair of the judgment. C. L. F.